cocks began to crow, the shadow now was inverted, and by degrees got smaller, until at nine o'clock the eclipse was over. I cannot but suppose that the scientific men must have had grand opportunities of observation, and that to-day's pencil will carry home many a description. Anything more beautiful, more sublime, or more perfect, it would be impossible to conceive.

Upway House, Mercara, Coorg, R. N. TAYLOR
Ooly, Dec. 12, 1871

The Rigidity of the Earth

I HAVE been urged from several quarters to defend my argument for the rigidity of the earth against attacks which are supposed to have been made upon it. It has, in fact, never been attacked to my knowledge, and I feel under no obligation to defend it. There is, I believe, a general impression that grave objections to it have been raised by M Delaunay, and it seems that even in this country some geological writers and teachers, in their reluctance to abandon the hypothesis of a thin solid crust, enclosing a wholly liquid mass, hastily concluded that all dynamical arguments against it had been utterly overthrown by Delaunay.

In point of fact Delaunay made no reference at all to the tidal argument, and clearly was unaware that I had brought it forward when he made his communication on the "Hypothesis of the interior fluidity of the terrestrial globe,"* to the French Academy, three years and a half ago, objecting to Hopkins's argument founded on precession and nutation, and merely quoting me as having expressed acquiescence. On this subject I say nothing at present, except that ten years ago, before I expressed (in my first communication of the tidal argument to the Royal Society) my assent to Hopkins's argument from precession and nutation, I had thought of the objection to this argument since brought forward by Delaunay, and had convinced myself of its invalidity. But I hope to be able on some future occasion to return to the subject, and to prove that any degree of viscosity, acting in the manner and to the effect described by Delaunay, must in an extremely short time abolish the distinction between summer and winter. My reason for writing to you at present is that I see in Mr. Scrope's beautiful book on Volcanoes (just published as a second edition) a sentence ("Prefatory Remarks," page 24), written on the supposition that the tidal argument had been brought forward for the first time at the recent meeting of the British Association in Edinburgh. I therefore take the liberty of suggesting to you that a reprint of the short abstract of my tidal argument, which appeared in the Proceedings of the Royal Society, for May 16, 1862, might not be inappropriate to your columns. I ought, however, to inform you that the tidal argument was carefully re-stated in the first volume of the treatise on Natural Philosophy, by Prof. Tait and myself, published in 1867, but as the volume is at present out of print, you may not consider this objection fatal to my proposal.

Glasgow University, Jan. 9 WILLIAM THOMSON

Abstract of Paper on the Rigidity of the Earth, by Prof. Sir William Thomson, F.R.S., received April 14, 1862

The author proves that unless the solid substance of the earth be on the whole of extremely rigid material, more rigid for instance than steel, it must yield under the tide-generating influence of sun and moon to such an extent as to very sensibly diminish the actual phenomena of the tides, and of precession and nutation. Results of a mathematical theory of the deformation of elastic spheroids, to be communicated to the Royal Society † on an early occasion, are used to illustrate this subject. For instance, it is shown that a homogeneous incompressible elastic spheroid of the same mass and volume as the earth, would, if of the same rigidity as glass, yield about $\frac{7}{9}$, or if of the same rigidity as steel about $\frac{2}{9}$ of the extent that a perfectly fluid globe of the same density would yield to the lunar and solar tide-generating influence. The actual phenomena of tides (that is, the relative motions of a comparatively light liquid flowing over the outer surface of the solid substance of the earth), and the amounts of precession and nutation, would in one case be only $\frac{2}{9}$ and in the other $\frac{2}{5}$ of the amounts which a perfectly rigid spheroid of the same dimensions, of the same figure, the same homogeneous density, would exhibit in the same circumstances. The close agreement with the results of observation presented by the theory of precession and nutation, always hitherto worked out on the supposition that the solid parts of the earth are perfectly rigid, renders it scarcely possible to admit that there can be any such discrepancy between them as 3 to 5, and therefore almost necessary to conclude that the earth is on the whole much more rigid than steel. But to make an accurate comparison between theory and observation, as to precession, it is necessary to know the absolute amount of the moment of inertia about some diameter; and from this we are prevented by the ignorance in which we must always be as to the actual law of density in the interior. Hence the author anticipates that the actual deformation of the solid earth by the lunar and solar influence may be more decisively tested by observing the lunar fortnightly and the solar half-yearly tides.* These tides, it may be supposed, will follow very closely the "equilibrium theory" of Daniel Bernouilli for all oceanic stations, and the author suggests Iceland and Teneriffe as two stations well adapted for the differential observations that would be required.

The earth's upper crust is possibly on the whole as rigid as glass, more probably less than more. But even the imperfect data for judging referred to above render it certain that the *earth as a whole must be far more rigid than glass*, and probably even more rigid than steel. Hence the interior must be on the whole more rigid, probably many times more rigid, than the upper crust. This is just what, if the whole interior of the earth is solid, might be expected when the enormous pressure in the interior is considered, but it is utterly inconsistent with the hypothesis held by so many geologists that the earth is a mass of melted matter enclosed in a solid shell of only from 30 to 100 miles' thickness. Hence the investigations now brought forward confirm the conclusions arrived at by Mr. Hopkins, that the solid crust of the earth cannot be less than 800 miles thick. The author indeed believes it to be extremely improbable that any crust thinner than 2,000 or 2,500 could maintain its figure with sufficient rigidity against the tide-generating forces of the sun and moon, to allow the phenomena of the ocean tides and of precession and nutation to be as they are.

Extract from Thomson and Tait's "Natural Philosophy."

"§ 832. . . . . All dynamical investigations (whether "static or kinetic) of tidal phenomena, and of precession and "nutation, hitherto published, with the exception referred to "below, have assumed that the outer surface of the solid earth "is absolutely unyielding. A few years ago, for the first time, "the question was raised: Does the earth retain its figure with "practically perfect rigidity, or does it yield sensibly to the de-"forming tendency of the moon's and sun's attractions on its "upper strata and interior mass? It must yield to *some* extent, "as no substance is infinitely rigid. But whether these solid "tides are sufficient to be discoverable by any kind of observa-"tion, direct or indirect, has not yet been ascertained. The "negative result of attempts to trace their influence on ocean "and lake tides, as hitherto observed, and on precession and "nutation, suffices, as we shall see, to disprove the hypothesis "hitherto so prevalent, that we live on a mere thin shell of solid "substance enclosing a fluid mass of melted rocks or metals, "and proves, on the contrary, that the earth is much more rigid "than any of the rocks that constitute its upper crust."

"§ 833. The character of the deforming influence will be "understood readily by considering that if the whole earth were "perfectly fluid, its bounding surface would coincide with an "equipotential surface relatively to the attraction of its own "mass, the centrifugal force of its rotation and the tide generat-"ing resultant of the moon's and sun's forces, and their kinetic "reactions. Thus there would be the full equilibrium lunar and "solar tides; of $2\frac{1}{2}$ times the amount of the disturbing deviation "of level if the fluid were homogeneous, or of nearly twice "this amount if it were heterogeneous with Laplace's hypotheti-"cal law of increasing density. If now a very thin layer of "lighter liquid were added, this layer would rest covering the "previous bounding surface to very nearly equal depth all round, "and would simply rise and fall with that surface, showing only "infinitesimal variations in its own depth, under tidal influences. "Hence had the solid part of the earth so little rigidity as to "allow it to yield in its own figure very nearly as much as if it were "fluid, there would be very nearly nothing of what we call tides "—that is to say, rise and fall of the sea relatively to the land; "but sea and land together would rise and fall a few feet every

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* *Comptes Rendus* for July 13, 1868.

† Communicated August 22, 1862, and read November 27, of same year "Dynamical Problems regarding Elastic Spheroidal Shells and Spheroids of incompressible Liquids."

* High tide, as far as the influence of either body is concerned, is produced at the poles, and low average water at the equator, when its declination, whether north or south, is greatest, and low water at the poles and high water at the equator, when the disturbing body crosses the plane of the equator.

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“ twelve lunar hours. This would, as we shall see, be the case if the geological hypothesis of a thin crust were true. The actual phenomena of tides, therefore, give a secure contradiction to that hypothesis. We shall see, indeed, presently, that even a continuous solid globe of the same mass and diameter as the earth, would, if homogeneous and of the same rigidity as glass or as steel, yield in its shape to the tidal influences three-fifths as much or one-third as much as a perfectly fluid globe; and further, it will be proved that the effect of such yielding in the solid, according as its supposed rigidity is that of glass or that of steel, would be to reduce the tides to about $\frac{2}{5}$ or $\frac{2}{3}$ of what they would be if the rigidity were infinite.”

“ § 834. To prove this, and to illustrate this question of elastic tides in the solid earth, we shall work out explicitly the solution of the general problem of § 696 for the case of a homogeneous elastic solid sphere exposed to no surface traction; but deformed infinitesimally by an equilibrating system of forces acting *bodily* through the interior, which we shall ultimately make to agree with the tide generating influence of the moon and sun. . . . .”

“ § 847. We intend in our second volume to give a dynamical investigation of precession and nutation, in which it will be proved that the earth's elastic yielding influences these phenomena in the same proportionate degree as it influences the tides. We have seen already that the only datum wanted for a comparison between their observed amounts and their theoretical amounts on the hypothesis of perfect rigidity, to an accuracy of within one per cent., is a knowledge of the earth's moment of inertia about any diameter within one per cent. We have seen that the best theoretical estimates of precession hitherto made, are in remarkable acordance with the observed amount. But it is not at all improbable that better founded estimates of the earth's moment of inertia, and more accurate knowledge than we yet have from observation, of the harmonic of the second degree in the expression of external gravity, may show that the true amount of precession (which is known at present with extreme accuracy) is somewhat smaller than it would be if the rigidity were infinite. Such a discrepancy, if genuine, could only be explained by some small amount of deformation experienced by the solid parts of the earth under lunar and solar influence. The agreement between theory on the hypothesis of perfect rigidity, and observation as to precession and nutation, are, however, on the whole so close as to allow us to infer that the earth's elastic yielding to the disturbing influence of the sun and moon is very small—much smaller, for instance, than it would be if its effective rigidity were no more than the rigidity of steel.”

“ § 848. It is interesting to remark that the popular geological hypothesis, that the earth is a thin shell of solid material, having a hollow space within it filled with liquid, involves two effects of deviation from perfect rigidity, which could influence in opposite ways the amount of precession. The comparatively easy yielding of the shell must, as we shall see in our second volume, render the effective moving couple, due to sun and moon, much smaller than it would be if the whole interior were solid, and on this account must tend to diminish the amount of precession and nutation. But the effective moment of inertia of a thin solid shell containing fluid, whether homogeneous or heterogeneous, in its interior, would be much less than that of the whole mass if solid throughout; and the tendency would be to much greater amounts of precession and nutation on this account. It seems excessively improbable that the defect of moment of inertia due to fluid in the earth's interior, should bear at all approximately the same ratio to the whole moment of inertia, that the actual elastic yielding bears to the perfectly easy yielding which would take place if the earth were quite fluid. But we must either admit this supposition, improbable as it seems, or conclude (from the close agreement of precession and nutation with what they would be if the earth were perfectly rigid) that the defect of moment of inertia, owing to fluid in the interior, is small in comparison with the whole amount of inertia of the earth about any diameter; and that the deformation experienced by the earth from lunar and solar influence is small in comparison with what it would be if the earth were perfectly fluid. It is, however, certain that there is some fluid matter in the interior of the earth; witness eruptions of lava from volcanoes. But this is probably quite local, as has been urged by Mr. Hopkins, who first adduced the phenomena of precession and nutation to disprove the hypothesis that the solid part of the earth's mass is merely a thin shell.”

The Kiltorcan Fossils

I HAVE just seen Mr. Carruthers' letter in your number of January 4th, to which I beg leave to reply.

In this communication it now appears that Mr. Carruthers' former remarks in the discussion upon Prof. Heer's paper were intended as a personal attack upon me; as he now states that on me alone rests the credit of misleading Prof. Heer by my erroneous determination of the Kiltorcan plant.

I have no hesitation in acknowledging to having referred the Kiltorcan plant in question to *Sagenaria veltheimiana*, and I think it very possible I may even now be correct. I will however now state the reason for my afterwards adopting Professor Schimper's name in preference. When that gentleman was in Ireland he spent some time in the examination of the Kiltorcan fossils, and did not then object to my determination of the species; it was afterwards, on my sending him a collection, that his further study of these fossils and comparison with the original species (of which I had only seen figures) enabled him to announce to me what he believed to be the distinctive characters in relation to the fruit which accompanied it, of those I had named *Sagenaria veltheimiana;* these fossils in his letter to me he referred to *Sagenaria*, and afterwards in his work “Traité Paléontologie Végétale,” to *Knorria* under the name of *K. bailyana*. In the meantime I had read my report on these fossils at the British Association, and naturally adopted the generic name first applied to it by Prof. Schimper, which I afterwards corrected to *Knorria*, on his authority, in my “Figures of British Fossils,” as Mr. Carruthers states.

In my letter to Professor Heer (June 1870) accompanying the specimens which I was requested to send him for his comparison with the Bear Island flora, I named those from Kiltorcan *Sagenaria bailyana* in accordance with Prof. Schimper's determination, whilst others from Tallow Bridge, co. Waterford, which he specially wished to see, I still referred to *S. veltheimiana.* I made him aware of Prof. Schimper's views on these plants, stating distinctly that they were originally referred by me to *S. veltheimiana*, but that Prof. Schimper, in consequence of his being enabled to compare the fruit accompanying it with that of the true *S. veltheimiana*, had arrived at the conclusion that it could not be that species, and therefore he had named it as a distinct one. Under these circumstances I cannot see how Mr. Carruthers can charge me with misleading Prof. Heer, who had the whole facts, with examples of the specimens from both localities, to draw his own conclusions from; with his acknowledged powers of discrimination, surely he was fully competent to judge for himself as to their correct identity.

The amount of Mr. Carruthers' knowledge on the subject about which he writes, is evidenced from his intimation that the fossil figured by me in the explanation to Sheet 187, &c., of the Irish Survey maps, is from Kiltorcan (co. Kilkenny), whereas it was sketched by me, on the spot, at Tallow Bridge (co. Waterford), where the section exposed exhibited a profusion of these plants in various conditions and stages of growth. The character of the rock in which they occur is totally different from that at Kiltorcan, the former being a grey shale, corresponding with the Lower Carboniferous shales, the latter a fine-grained greenish sandstone; neither has any of the associated Kiltorcan fossils, including the fish which are of typical Devonian or Old Red sandstone genera, ever been found at Tallow Bridge. I did however state in this memoir my belief that the *S. veltheimiana*, as identified by me at Tallow Bridge, was similar to the Kiltorcan plant in question, and also that it corresponded with the so-called *Knorria* of the Marwood beds, N. Devon.

With reference to Mr. Carruthers' announcement that *Sagenaria veltheimiana* is a “coal measure plant,” I may remark that it is a particularly abundant fossil, occurring in various conditions, but seldom, if ever, met with in the typical coal series of Great Britain; I have identified it from the sandstones of the lower coal measures in the North of Ireland, as well as at various localities in the Lower Carboniferous shales of the Counties of Cork and Kerry. On the Continent, especially in Germany, it appears to be still more universal, and has been recorded under various names by fossil botanists, as Dr. H. R. Goeppert, in his “Fossile Flora der Silurischen der Devonischen,” &c., mentions more than twenty synonyms for this species; moreover the same author states its occurrence to be “In der Kulmgrauwacke, dem Kohlenkalke und in der *jungsten Grauwacke*.” Dr. F. Unger and Dr. H. B. Geinitz, the latter of whom personally inspected the collections from Kiltorcan and Tallow Bridge, also mentions similar lower geological horizons at which it occurs; and Dr.

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